

FYSIKALISK-KEMISKA INSTITUTIONEN
UPSALA

October 20. 1936.

TELEFONER: 5555 · 5556 · 5557

KOP/IP

MH 76 F13

PS. Do you think I may get a copy of the MS when you have send it to the editor. I am very anxious to know the improvements you are going to make and to hear what you think of the final experimental results.
K.O.P.

Dear Professor Heidelberger.

Yesterday I mailed the MS to you (as registered letter) and this morning we have sent your handwritten MS together with a summary of the Rabbit and Horse Sera runs beside a copy of the two most characteristic sedimentation diagrams from each run (this was mailed as registered "affärshandlingar" this morning). I hope you will get these at the same time as this letter, otherwise they must come shortly after. For the case you have not got the first letter I add a copy of this.

As regards the MS I reckon that you are going to bring it into its final shape, so I have not at all bothered about getting it into the correct language, and I hope that you will change ^{it} in all the ways which you think advisable. - The MS has come out rather long, but I think it difficult to shorten it much without excluding things that are valuable for the understanding of the experimental results. I was a little in doubt about the best way of stating the experimental results, since I was afraid that the tables would not give a correct picture of the centrifuge experiments. Finally I arrived ^{at} these diagrams, and I think that they really give a fairly good picture of the results. Svedberg and Tiselius said that it was a great advantage for them to have them, when they were reading the MS. - As to the typographical performance of these diagrams they will have to be rather much diminished, probably at least to 1/3 scale. The lines for the rectangles must be heavily drawn in order

that they may be easily distinguished from the text. It is probably best to print the text in the diagrams. The diagrams I send you are just rough-draughts in order to save time, if I should have got the fine drawn done it would probably have meant a delay of another week, since our drawer from now until Christmas is piled up with work, but I hope you can get it done easily in New York.

I have been a little afraid whether the MS should be more than the 25 printed pages they state as a maximum in J.Exp.Med. But do you not think that all the description of the experiments could be printed with "petit", I think so. If it is still too long, do you think it would be possible to divide it into two papers? The one dealing with the rabbit experiments and the other with those done on horse sera.

You will probably observe that the new sedimentation constants given in several cases are different from the ones that were calculated when you left. Slight changes may be due to the fact that we have recalculated most of the runs and in some cases we have got additional exposures compared. But the main cause of change is due to the additional correction which I have applied on these experiments. It is a very difficult problem in what manner to correct the sedimentation constants in the more concentrated solutions and in the mixtures (f.inst. serum). I am working on the problem and hope to come to some conclusion sooner or later, but so far I have not got any definite results. In lack of results I have used the same method as v.Mutzenbecker used for correcting his sedimentation constants for serumalbumin and serumglobulin to infinite dilution, viz. by multiplying the (for salt and temp. corrected) s_{20} with the viscosity increase due to the protein itself. That means I have multiplied the salt-corrected s_{20} by $\frac{\eta_{\text{protein}}}{\eta_{\text{buffer solution}}}$, where η_{protein} is the viscosity of a protein solution having the same concentration and composition as the centrifuge solution has where the molecule

corresponding to this s , sediments; or in another way: s_1 has been corrected for the protein concentration c_1 , s_2 for protein concentration $c_1 + c_2$ and s_n has been corrected for the protein concentration $c_1 + c_2 + \dots + c_n$. The values for η for ovalbumin, serumalbumin and serumglobulin have been taken from determination of the viscosity in pure solutions of these proteins, whereas the η values for the faster sedimenting substances have been put equal to the values for serumglobulin.

The values for s_{20} are the values which are corrected in the way just described, and these values are given in the diagrams.

In my earlier letters I have not had time to answer some of your questions in your letters of September 4. and September 13. I shall try to do this now.

First so very many thanks for the nice blue thermosbottle which was used for sending the serum samples from London to Upsala. We have used it already several times and enjoyed it very much; it followed us on a weekend trip in the canoe in the middle of September. As to the 10 sh stamp, I had given it to my mother-in-law before I got your letter, where you asked me what Mr. Lönn thought about it. My mother-in-law is very glad to get stamps, so we always collect our stamps for her.

As to the runs with the polysaccharide (expt. 23 and 28), the result could be summarized as follows: In the experiment with the 1% solution (23) we got a very sharp peak, whereas in the dilute solution (expt. 28) the sedimentation started with a peak, but this peak became broader and broader ~~and~~ almost disappeared after a while. Even with the very long scale distances it was impossible to get any definite peak on the curves. This behavior is very typical for the "thread" molecules. Even in very polydisperse solutions one gets only a single and sharp boundary, when the concentration of the thread molecules is not very dilute. This state of affairs has been treated by Signer and Gross

(Helv.Chim.Acta 17, 59, 335 and 726 (1934)) for the polystyrol systems. The polysaccharide is therefore after all quite polydisperse. I wonder whether an osmotic pressure measurement will tell so very much in this case, since the polysaccharide undoubtedly must be very polydisperse, and I am afraid the osmotic pressure 'mean value' for M even may come out with the wrong order of magnitude, as the osmotic pressure measurement often does with the thread molecules. A diffusion experiment will be of little value too in this case.

It is very interesting that our experimental results show that we chose the right thing, when we decided to carry out the experiments in moderate dilute solutions. If we had carried out the experiments in concentrated solutions as McFarlane would suggest, then I am afraid that the result of the whole investigation would have been very meager. I have often told him that it is wrong to stick to a single concentration.

I think it must be a mistake when you speak about the $s_{20} = 2.8 \cdot 10^{-13}$ for EA in Experiment 21. The value is quite normal, but on the whole the values for the EA are not so very sure, because we often did not run the centrifuge long enough.

I add some proposals for text for the Figures on a separate sheet.

I have had piled up with work the whole autumn, and it has been even worse after the return of Professor Svedberg from U.S.A. He was very enthusiastic about his journey. He was very interested in the work done with the air-driven centrifuge at the Rockefeller Institution, and I think that ^(perhaps) there will be some sort of cooperation between the two institutes. Dr. Baur is probably going to write a chapter about the air-driven centrifuge in the centrifuge monography.

With the kindest regards to you, Mrs. Heidelberger and Charlie from Ida and

yours sincerely Kai O. Pedersen.

P.S. ✓ should ask you to order 200 reprints for our Institute (150 for the lab and 50 for me) and perhaps you could send us a list with the names of the people you are sending reprints to yourself in order that we are not sending to the same persons.